Among moulds, certain species of Mucor exist as several races the individuals of each of which reproduce themselves asexually, but do not conjugate with one another. When, however, individuals of different races meet, they conjugate and produce zygospores. It may be supposed that one race is of such a type as MM*ff*, another of the *mm*FF type. In this case verification of the hypothesis is possible.

The absence of sexual reproduction in various groups of fungi is to be explained on the present hypothesis as due to the extinction (or effective separation) of all zygotes except those of one type, *e.g.* the M*f* or the *m*F types.

In homosporous ferns, the spores, produced after the reduction division, give rise each to a prothallus which bears male and female organs. If it be allowed that the reduction division is of fundamental significance with respect to the segregation of characters, it would appear to follow that current Mendelian theories of sex-heredity fail to account for the fact that a spore produced as a consequence of the reduction division may yet carry "male" and "female" factors.

The phenomena may be interpreted simply in terms of the new hypothesis. The fern plant is MMFF; the spore, and hence the prothallus, carries MF. Therefore male and female organs may be produced by the prothallus. The gametes formed and matured in the female organs are "female," those formed and matured in the male organs are "male."

In the heterosporous ferns the spores are of two kinds, macrospores, giving rise to "female" prothalli, and microspores, which give rise to "male" prothalli. In terms of our hypothesis the sporophyte (zygote) is M*m*F*f*, the megaspore *m*F, and the microspore M*f*.

Further, the high rate of mortality which accompanies spore-formation receives on this hypothesis an intelligible explanation. It is due to the inevitable reappearance of combinations of sex-characters which the heterosporous fern has ceased to tolerate.

In the light of the present hypothesis, homosporous ferns are homosporous because they are homozygous, and heterosporous ferns are heterosporous because they are heterozygous for the sex characters M and F.

The significant question arises, How far is the present limitation of characters presented by any great group of organisms determined by the fact that in this group the task of reproduction has come to be committed to some particular type or types of gametes?

The hypothesis would appear to throw light on large numbers of known facts, on prepotency, partial sterility—such, for example, as occurs in heterostylism—the apparently excessive production of pollen and ovules, and so forth.

Not only is it not repugnant to a reasonable explanation of many facts, but also the hypothesis does not seem to be inherently improbable. In that it is based on the presence and absence theory, it receives the sanction of Mendelism. It tempts the imagination to trace the origin of sexuality from the "self-contained" organisms of the MF type. Evolution in such types took, in some individuals, the form of a dropping out of the M, in others, of a dropping out of the F, factor. Such incomplete forms as M*f* and F*m* discovered in fusion the means of restoring their constitutions; but out of this fusion possibilities for novel constitutions arose, for the M*m*F*f* type of zygote was now in being. In reproducing by segregation the original MF type of gamete, the zygote was constrained to produce likewise the other possible combinations of M*m* and F*f*. Fusions between the several types resulted in different forms of zygote; evolution had its chance.

Among other types, the pure recessive, *mmff*, arose, and, with its advent, sterility, and, it may be, death, came on the scene as the sinister shadow of "sexual" reproduction.

It only remains to add to this note that—in case the hypothesis it proposes prove of value—though the responsibility for the hypothesis rests with the writer, the stimulus to which it owes its inception originated, in the first place, from a study of Bateson's work on heredity, and in the second place from discussions on the problems of heredity between the writer and his colleagues, Miss Rayner, Mr. Jones, and Miss Pellew, of the botanical laboratory, to whom certain of the foregoing illustrations are due.

University College, Reading. FREDERICK KEEBLE.

Geology and the Earth's Axis of Rotation.

FROM time to time the pages of NATURE contain references to the theory which would explain the occurrence of Ice ages by a hypothetical shifting of the earth's axis of rotation. On the face of it, the theory in question appears to be capable of explaining a good deal more than this.

In the first place, if the axis of rotation were to be shifted, it seems clear that the relations between the earth's hydrosphere (or hydrospheroid) and the lithosphere must undergo change. In the regions towards which the pole is approaching land will tend to emerge from the sea, and *vice versa*. If the effects of this supposition be traced out in detail, they will be found to furnish an explanation of such phenomena as raised beaches, submerged river valleys, varying continental connections, &c., without postulating violent alterations in the lithosphere itself. Speaking merely qualitatively, the hypothesis seems to fit the facts pretty closely, *e.g.* (*a*) the height of raised beaches tends to increase as one approaches the polar regions, as it ought; (*b*) a marine transgression is associated with a warm climate.

In the second place, a shifting of the polar axis will not be without effect on the lithosphere itself, although such effect would not, presumably, under present conditions, at all resemble the effect on the hydrosphere already alluded to. Even in a rough qualitative way this effect is not easily traced out, but it seems tolerably clear that it will account for those processes of folding, &c., whereby mountain chains are built up, and also for extensive local subsidences such as are believed to have occurred in geological time. These, and doubtless other phenomena, the hypothesis explains without having recourse to the supposition that the earth has been undergoing contraction through loss of heat.

I am not aware of the existence of any publications dealing with the matters referred to, but as the subject appears to be not without interest, perhaps some of your other readers will be able to refer me to papers, &c., treating of the subject with which they may be acquainted. I should be particularly glad to be referred to researches in which the subject is treated quantitatively.

HUGH BIRRELL.

Holyrood House, Bo'ness, Linlithgowshire, N.B.,
February 4.

Secondary Cells in Tropical Climates.

ALL who have used batteries of small secondary cells in the tropics will have experienced the difficulty of keeping their cells in efficient working order, and especially in preserving the junction of separate cells from rapid corrosion. The difficulty, appreciable in Europe, becomes very serious in a climate where the laboratory temperature lies between 30° and 40° C., and for this reason—it is probable that practically all accumulators sent to tropical countries by European manufacturers are filled by their recipients with dilute sulphuric acid of a density (1,190) which corresponds to a 20 per cent. mixture in north Europe at 15° to 20° C., but at a temperature of 30° to 35° C. indicates a mixture which is far too rich in acid for the health of the cells. Some simple experiments recently carried out in this laboratory exhibit quite clearly how large a deviation from the standard 20 per cent. mixture is caused by filling cells at 30° with dilute acid of density 1,190. It is found that a density of 1,190 at 30° corresponds to a composition of 23 per cent., whereas the value of the composition accepted as giving the best results with cells of this type is 20 per cent. The difference is as much as half the total change in composition due to chemical action during the process of charging the cell.

The conclusion reached from an examination of the density-temperature curves for dilute sulphuric acid points to the advisability of filling all secondary cells in localities where the average temperature is 30° or more with acid solution of density about 1.170. Densities as low even as 1,150 have been found satisfactory for small secondary cells in the hot weather in Calcutta.

In the case of large plants in power stations, the matter may be still more important, as a cell containing too strong

an acid solution is likely, not only to live a shorter life, but to suffer sooner and more severely from sulphating and other diseases. E. P. HARRISON.

The Physical Laboratory, Presidency College, Calcutta, February 2.

The Invention of the Slide Rule.

DR. ALEXANDER RUSSELL'S remarks on the invention of the slide rule (NATURE, January 13, p. 307) are of great interest, particularly his reference to Seth Partridge. There can be no doubt that Partridge deserves much credit for improving the rectilinear slide rule, but I see no escape from the conclusion that the real inventor of the rectilinear slide rule is the one who first made two Gunter's scales to slide together, for purposes of computation. The man who did this is Oughtred. In Mr. Sidney Lee's "Dic. of Nat. Biog.," article "Partridge, Seth," and in other publications, the incorrect statement is made that Partridge's book, "Description, &c., of the Double Scale of Proportion," first appeared in print in 1671 or 1672. I have a copy of the book bearing the date 1662. The manuscript was finished "Saturday night, August first 1657." In 1662 Partridge's rules were manufactured, not by Walter Hayes, but by "Anthony Thompson, living in Hosier-Lane near West Smithfield, in London."

There is another point of interest. The earliest account of the rectilinear slide rule, printed in Germany, is in Leupold's "Theatrum Arithmetico-Geometricum," Leipzig, 1727, p. 71. Leupold says that he had a manuscript of ten sheets, describing it, but that he did not know the name of the author or the inventor of the instrument. Leupold's description consists of translation, word for word, of extracts from Partridge's book. Thus a historic connection is established between the rectilinear slide rule in England and in Germany. FLORIAN CAJORI.

Colorado Springs, Colorado, February 7.

Aged Tadpoles.

THE experience of your correspondent Mr. John Don (February 17, p. 458) is no new one. More than twenty years ago we had in a small aquarium in the Charterhouse Museum a tadpole two years of age. To the best of my recollection this veteran never acquired any legs, either hind or front, but the head and body were extraordinarily large. At the present moment I have in my laboratory seven living tadpoles reared from spawn deposited last spring. Of these, three only have developed hind legs. These appendages appeared rather suddenly in December, a few days after I had supplied, for the first time, some fragments of hard-boiled egg.

The secret of procuring these aged tadpoles is to keep the animals in a vessel with vertical sides, and to afford as little opportunity as possible for them to wriggle into shallow water. I maintain a depth of about 5 inches of water in the aquarium, and find that a subdued light favours the health of the tadpoles. Sexual maturity can hardly be expected until at least the normal period has elapsed, viz. in the third or even fourth year.

OSWALD H. LATTER.

Charterhouse, Godalming, February 18.

Title of the Natural History Museum.

IN NATURE of February 17 you say (p. 465):—"No one, so far as we know, has suggested a suitable and adequate title for the [museum] at South Kensington" (devoted to natural history). This is not a difficult question; I think "British Museum of Natural History" is both suitable and adequate. BERNARD HOBSON.

Tapton Elms, Sheffield, February 18.

THE NEW CANALS OF MARS.

THE word "new" when applied to a celestial phenomenon may be used in either of two senses. It may mean new to earthly observation, *i.e.* one which has never been seen by human beings before, or, secondly, new in itself, that is, one which has had no previous existence. New canals on Mars in the first sense, though always interesting, and at times highly important, are no novelty at this observatory, inasmuch as some four hundred have been discovered here in the last fifteen years. When Schiaparelli left his great work, he had mapped about 120 canals; with those detected here since, the number has now risen to between five and six hundred. Each of the four hundred thus added to the list, however rich an acquisition at the time it first came to be noticed, was not necessarily otherwise remarkable.

To observe, however, a canal new in the second meaning of the word, one, that is, that had never existed anteriorly, and to prove the fact, is an astronomical detection of far-reaching significance for the bearing it has upon the whole Martian question.

On September 30, 1909, when the region of the Syrtis Major came round again into view, after its periodic hiding of six weeks due to the unequal rotation periods of the earth and Mars, two striking canals were at once evident to the east of the Syrtis in places where no canals had ever previously been seen. Not only was their appearance unprecedented, but the canals themselves were the most conspicuous ones on that part of the disc. They ran one from the bottom of the Syrtis (lat. 20° N., long 285°), the other from a point part way up its eastern side (lat. 17° N., long 284°), and, curving slightly to the left as they proceeded south, converged to an oasis, itself new, on the Cocytus (lat. 5° N., long 265°), about two-thirds of the distance to where that canal meets the Amenthes. The Amenthes itself was not visible, except possibly as a suspicion. With the two main canals were associated several smaller ones, and at least two oases which had never been seen before, and from the interconnection of all of them these clearly made part of the new piece of Martian triangulation.

The phenomena were recorded in many independent drawings by Mr. E. C. Slipher and the director, and in the course of the next few days were photographed, appearing on the plates to the eye as the most conspicuous canals in the presentment of the planet. It is opportune that detailed photography of Mars in Mr. Lampland's skilful hands should have been so perfected as to make this possible; for the photographs taken by both Mr. E. C. Slipher and the director record these canals so that anyone may see them. There are thirty images, more or less, on each plate, and the canals appear on every image; on some more distinctly than on others, owing to the state of our air at the time, but recognisably on all; for each image had a pose of about two seconds and a half, and its definition varied according to the seeing at the time. Owing to the grain of the plate being much coarser than that of the eye, the two canals appear merged in one in the photographic images as a single line, its linear character, however, being quite distinct to one of good eyesight.

The photographs of this region taken in 1907 show no such feature.

No remembrance of ever having seen them before could be recalled by either observer, both being familiar with the planet, except that Mr. Slipher turned out to have drawn one of them the evening previous.

The record books were then examined, when it appeared that not a trace of them was to be found in the drawings of August, July, June, or May when this part of the planet was depicted. That they had not been observed in previous years was then conclusively ascertained by examination of the records of those years. The record of canals seen here is registered after each opposition in a fresh map of the planet's surface. This has been done since the beginning of the critical study of Mars at this observa-